This study presents an integrated in situ observations of high-resolution intra-tidal bed-level evolution and near-bed suspended sediment concentration (SSC) on a macrotidal dissipative beach, obtained using an Argus Surface Meter (ASM) during and after Typhoon Doksuri. The authors investigate bed-level responses through successive inundation–exposure cycles and compare the processes occurring during extremely shallow-water and relatively deep-water stages. These valuable observations help improve understanding of the different processes operating in the intertidal zone.

However, some of the proposed mechanisms require more rigorous explanation before potential publication. The most critical issue is that the authors infer ESWS mechanisms from bed-level and SSC observations, but some interpretations go beyond what the available data can support. The discussion should therefore focus more closely on the observed morphological changes and associated processes. With careful revision, this study could make a meaningful contribution. Specific Comments are below：

Major Issues

1. The title is complicated and could be simplified.
2. The Introduction provides a relatively brief review of previous research on the ESWS. Although relevant tidal-flat studies are cited, their main findings and remaining questions are not sufficiently summarized. The authors should further discuss existing knowledge of ESWS hydrodynamics and associated erosion–deposition responses. I recommend citing studies published within the last five years on related topics and expanding on representative case studies, so as to highlight recent research progress and its remaining limitations. What is the basis for using a water depth of 0.3 m as the threshold between shallow and deep water? In addition, the ESWS and RDWS stages may control beach stability through different sediment contributions, and the authors state that quantitative understanding of this mechanism is lacking. Is it truly the case that no published study has conducted a quantitative analysis of either one of these stages or of both? I suggest substantially expanding the review of progress in quantitative research, from which the unique contribution of the present study can be clearly identified, while avoiding overly definitive statements. Finally, the authors claim that high-resolution continuous measurements capable of simultaneously capturing bed elevation and sediment dynamics over the full tidal range are extremely rare. If so, the Introduction should at least include a review of studies that have measured one of these aspects alone (bed elevation or sediment dynamics), so that the existing gaps can be summarized and distilled, thereby naturally leading to the measurement system proposed in this study.
3. Study site and description of limitations. The authors should explain why a beach on the southern part of Pingtan Island, Fujian, was selected as the study site. In addition, the applicability of the Argus Surface Meter (ASM) should be clearly defined, specifically to which other beaches, and to which beach types beyond those in Fujian, the instrument is suited. At the same time, the conditions under which the ASM is not applicable should also be explicitly stated.
4. The main novelty of this study lies in ESWS, yet hydrodynamic observations, such as velocity, shear stress, infiltration, or groundwater level, are unavailable for this stage. Statements such as “ESWS maintains continuous and efficient sediment transport” and “a

complete ESWS process is critical for stability" are plausible but need stronger evidence. The authors should reframe these statements as inferences based on the observed bed-level and SSC patterns.

5. Section 5.1 discusses cross-shore sediment transport primarily on the basis of SSC and current direction, but changes in sediment conditions require stronger supporting evidence. For example, differences in the SSC–τ_w regression slope and between flood and ebb tides can reflect changes in suspension responses, but they are insufficient to directly establish that the water column is sediment-undersaturated.
6. How was SSC at 0.03 m above the bed determined?

Minor Issues

1. Section 5.2 suggests that muddy beds have greater erosion resistance under similar wind speeds. This statement is not generally valid: it may hold under wind-wave conditions, but may not apply under swell conditions. Please specify the conditions to which this comparison applies.
2. In Section 5.3, the assertion that a complete ESWS process is necessary for stability is intuitive but has not been fully demonstrated.
3. Lines 279–282: The comparison with wave-dominated beaches is useful but brief. Please expand the discussion slightly to highlight the key differences.
4. Figure 1 lacks a scale bar. Figure 3 is somewhat unappealing in its current form and should be improved or moved to the supplementary materials. The font in Table 2 should be changed to Times New Roman (TNR). I also suggest presenting the conclusions in a point-by-point format.
5. The reference list as a whole appears somewhat outdated. I recommend adding recent studies from the past five years, which is very important for the topic of this paper.